

Interactive comment on “Simulations over South Asia using the weather research and forecasting model with chemistry (WRF-Chem): chemistry evaluation and initial results” by R. Kumar et al.

Anonymous Referee #1

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This is a counterpart of the first paper of the same authors with subtitle, “set-up and meteorological evaluation,” and presents the validation of chemical species, CO, NO₂ and O₃. The paper is presenting comprehensive regional view of ozone and its precursors in South Asia. It is in general well written but there are some suspicious points, which should be cleared before the publication.

Some of the comments that would be useful for revision are as follows.

1. p.15 & Fig.4 Ozone seasonality plots should be provided not only in the form of ΔO_3 but also in the absolute concentrations. It is very informative if the model gives the characteristics of either overestimate or underestimate of O₃ mixing ratio. Although

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there are not many papers showing the comparison between the model output with observational data in India, Ellingsen, et al, (Atmos. Chem. Phys. Discuss., 8, 2163–2223, 2008) shows much of overestimate by global models for N. India. Since this issue is also related to the suspected underestimation of NO_x emission as compared to the satellite data discussed later, some discussion is necessary here on the comparison of absolute concentration.

2. p.15-16 Although it is evident that the summer low of ozone in India is mainly due to the intrusion of the pristine marine air just as same as in East Asia, the problem here is that the discussion is too much focused on cloud and rainfall as a factor of reducing ozone mixing ratio. Even though cloud would reduce the local photochemical ozone production in wet season, it is not the major reason to give low ozone either at coastal and inland sites, and the major reason is the long-range transport of oceanic air. It should be noted that “rainfall” repeatedly mentioned in the manuscript as a cause of low ozone due to suppressing photochemical production is not correct and suggested to be removed. It has been well established that ozone itself is not scavenged by rain at all, wet scavenging of NO₂/HCHO would not be very effective to affect O₃, and the wet removal of HNO₃ is not sensitive to O₃ concentration since photolysis rate of HNO₃ is too slow to affect local O₃. If the authors are interested in, it may be worthwhile to evaluate the impact of wet scavenging of NO₂/HCHO and HNO₃ on local O₃ concentration.

3. p.17 and Fig. 5 Although it is mentioned that “The variations in MOZART CO and NO_x values are similar to WRF-Chem except for NO_x variations at Mt. Abu”, Fig. 5 shows, the agreement between MOZART and WRF-Chem for NO_x at Mt. Abu is as good as others. Instead, WRF-Chem ΔNO_x in January is unusually high. If the data is correct, what gives this anomaly? Further, discrepancy between the WRF-Chem data giving much higher values in February-April at Gadanki, and observation/MOZART data should be discussed in the text.

4. p. 23-27, Fig. 13, 15, & 16 Underestimate of NO₂ tropospheric column density by

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WRF-Chem against OMI and GOME-2 by a factor of 2-3 (up to 5) is puzzling. If it is real and could be ascribed to the underestimate of NO_x emission, it is interesting to see how much O₃ concentrations would be increased by the increase of NO_x emission say by a factor of 2. In this regard, model comparison with surface O₃ data in absolute concentrations is interesting and should be mentioned as noted in comment 1 above, although comparison with TES retrieved O₃ is presented in Fig. 8 in surface-500 hPa.

5. p.27-31, Fig. 17 Fig. 17 shows a very peculiar spatial distribution of O₃ by season in India. As discussed in the paper, summer low of O₃ is reasonable, but it is very unusual that surface O₃ concentration in spring is lower than winter and autumn. There is some suspicion in the figure that O₃ in Burma in spring where NO_x and CO concentrations are higher than other seasons due to biomass burning (Fig. 9 & 13), is even lower than in winter. In winter and autumn some coastal oceanic area of India surface O₃ concentrations are even higher than the inland. This is very strange since even the outflow of O₃ is efficient in these seasons, O₃ net production is much higher in the terrestrial area than the oceanic area as shown in Fig. 18. Therefore, the concentration of O₃ in the outflow region is expected to be lower than the source region in general. Some explanation after checking the data would be necessary.

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